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Fiscal News, State Budget Rules, and Tax-Exempt Bond Yields[1]

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This paper investigates how state fiscal institutions such as balanced-budget rules and restrictions on state debt issuance mediate the bond market reaction to state fiscal news. We analyze data on the yields of bonds issued by different states, as reported in the ''Chubb Relative Value Survey,'' along with data on state budget forecasts for the period 1988–1998. We find that unexpected deficits are correlated with higher state bond yields. This effect is smaller for states with tight antideficit rules than for states without these fiscal rules. Unexpected deficits have a particularly large effect in raising bond yields of states with tax limits. These results suggest that bond market participants view fiscal institutions as relevant in assessing the risk characteristics of tax-exempt bonds and that the economic significance of these institutions depends on the state's economic and fiscal circumstances. © 2001 Academic Press

During the past decade, U.S. states have experienced dramatic changes in their fiscal circumstances. In the early 1990s, many states faced fiscal difficulties, and their borrowing costs increased sharply. There have been wide interstate differences in general obligation debt yields. In 1990, for example, Massachusetts was paying more than 100 basis points more on general obligation debt than many other Northeastern states. The relative yields on debt

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issued by California and New York have varied by nearly 60 basis points. The wide disparities in state borrowing costs reflect divergent economic conditions, but they may also reflect more systematic factors such as differences in state political institutions or fiscal constitutions.

The effect of fiscal institutions on fiscal policy and economic performance more generally attracted substantial policy attention and academic research in the past decade. Discussions leading to the creation of the European Monetary Union, described for example in Von Hagen [28] and Inman [15], and U.S. fiscal experiences with federal budget deficits in the early 1990s were important stimuli to this work. An important issue in this literature is the extent to which credit markets evaluate the credit worthiness of sovereign borrowers, thereby indirectly providing a source of fiscal discipline. The notion that participants in sovereign debt markets form sophisticated views of the fiscal prospects for different jurisdictions offers an intriguing strategy for testing a range of hypotheses about the economic effects of fiscal limits. When jurisdictions differ in their fiscal rules, empirical research could in principle identify the effect of different provisions in the fiscal constitution on borrowing costs. Several previous studies, including Eichengreen [9], Goldstein and Woglom [13], Bayoumi, Goldstein, and Woglom [4], Lowry and Alt [17], and Poterba and Rueben [24], have applied this strategy to the U.S. states. The results in this literature suggest that state fiscal institutions have some explanatory power for the level of borrowing costs.

The potential endogeneity of state fiscal rules is an empirical challenge to any study of fiscal institutions and borrowing costs. It is possible that states with tight antideficit rules also have fiscally conservative electorates and that the primary factor explaining their lower borrowing costs is the electorate, not the fiscal rules. Previous efforts to find instrumental variables that are correlated with fiscal institutions, but not with fiscal policy and borrowing costs, have met with only limited success.

This paper tackles the endogeneity problem by focusing on *changes* in state borrowing costs, rather than on the level of these costs. It considers the reaction of tax-exempt bond yields to news about state fiscal conditions, particularly unexpected state deficits and surpluses. The core of the empirical analysis is a study of how state fiscal institutions mediate the bond market reaction to fiscal news. The assumption that motivates this analysis is that changes in fiscal conditions are less likely than the level of fiscal variables to be correlated with state voter preferences. Our study is designed to provide a new source of evidence on the economic effects of fiscal rules, while avoiding at least some of the endogeneity problems that may have affected earlier studies.

Our results are complementary to those in Lowry and Alt [17], who estimate models in which the state's borrowing cost depends on the current level of the state deficit, and the interaction of this deficit with the state's fiscal rules. They find that deficits matter less for bond yields in states with tight fiscal constitu-

tions than in states with looser fiscal rules. Their analysis is based on bond yields for the 1973–1990 period. Our analysis focuses on deficit shocks, not deficit levels, and relies on data for the 1988–1999 period. The parallel between our findings and theirs is reassuring, and it suggests that our results are not simply an artifact of the recent period for which budget ''shock'' data are available.

This paper is divided into five sections. The first presents our model of yield determination in the tax-exempt bond market and explains how we view fiscal institutions as affecting yields. Section 2 describes our framework for estimating the ''fiscal shock'' experienced by different states in different years. The third section summarizes the state fiscal institutions that we consider, and it reports descriptive statistics on the level of bond yields in different states. Section 4 presents our principal empirical findings. It focuses on results using a 10-year panel of borrowing costs and fiscal institutions for a set of 39 states. The last section summarizes our findings and suggests promising directions for future work.

1. EQUILIBRIUM IN THE TAX EXEMPT BOND MARKET AND THE IMPACT OF FISCAL SHOCKS

The theoretical model of yield determination in the tax-exempt bond market that underlies our empirical work assumes that the yield on bonds issued by each state generates the same after-tax return for ''marginal investors'' as does holding taxable Treasury bonds. This is the model that underlies most previous empirical work on state taxes, fiscal rules, and yield determination. The following presentation draws on Poterba and Rueben [24], although it differs in assuming that a Treasury bond, rather than a corporate bond, is the alternative to the tax-exempt state bond.

If R_{Tt} denotes the Treasury yield, and R_{it} denotes the tax-exempt yield on bonds issued by state i in period t, then asset market equilibrium requires that R_{it} equal the after-tax Treasury yield plus a risk premium. This can be written

$$R_{it} = \left[1 - \tau_{f,i}(B_t)\right] R_{Tt} + \sigma_i(Z_{it}, X_{it}, B_{it}, C_{it}). \tag{1}$$

In this expression, $\tau_{f,i}$ denotes the marginal federal income tax rate on interest income for an investor in the bonds issued by state i, and B_t represents the aggregate stock of tax-exempt debt outstanding. There are no state income taxes on the interest paid on Treasury bonds. The risk premium on tax-exempt debt issued by state i (σ_i) depends on four sets of variables. The first is Z_{it}, a vector of state fiscal institutions that affect the expected future supply of tax-exempt debt from state i and the probability of future payment of current interest obligations. The second is X_{it}, a set of state-specific economic and fiscal factors, such as the unemployment rate and the state budget deficit, that affect the probability that the state will be able to repay its obligations. The

third is B_{it}, the outstanding stock of debt issued by state i. An increase in the outstanding debt stock might reduce the marginal federal tax rate of the ''marginal investor'' holding state bonds. It may also provide information to bond market participants on the likelihood that the state will be able to pay the promised interest on newly issued bonds.

The final variable that affects the risk premium is C_{it}, which is a measure of the state's ''fiscal taste.'' The effect of this ''fiscal taste'' variable on the risk premium is difficult to evaluate ex ante since a fiscally conservative state might not raise future taxes to cover interest payments, but it also might be less likely to incur deficits than a less conservative state. State fiscal institutions are likely to be correlated with state fiscal taste, but since we do not observe fiscal taste, we need to develop an empirical strategy in which our estimates will not be contaminated by this omitted variable.

Equation (1) assumes that *risk premia* can explain interstate differences in borrowing costs. One unresolved question is whether state debt is risky enough to justify such risk premia or whether other factors are needed to account for interstate differences in borrowing costs. Although state defaults are rare, they have occurred. English [10] discusses nineteenth century defaults by U.S. states in some detail; most of these were the result of aggressive state borrowing to develop unprofitable canal systems in the 1830s and 1840s. The link between the outstanding debt stock and the risk premium is straightforward. A larger debt burden, relative to the state's economy, corresponds to a greater risk of being unable to meet interest obligations. The link between economic conditions and the risk premium is also clear: for a given debt stock, the larger the economic base in the state, ceteris paribus, the lower the chance that the state will default. This is because a larger economy generates a larger tax base.

Fiscal institutions (Z_{it}) can affect the risk premium on state bonds for several reasons. Rules that make it more difficult for states to raise taxes increase the likelihood of future default on promised interest payments. Antideficit provisions in state constitutions and rules that limit the power of the legislature to issue new debt may affect the future supply of state debt and therefore reduce the chance that the future supply of debt will expand and drive down bond prices.

While the stock of debt outstanding can affect a state's risk premium, the stock of debt outstanding may also *be affected* by the prevailing interest rate on the state's bonds. Metcalf [18] models the debt issue decisions of states and finds that more debt is issued when interest rates are lower. The interactions between the debt stock and the interest rate complicate our empirical analysis. By focusing on changes in yields in response to unexpected fiscal shocks, we hope to avoid the simultaneity in the determination of yields and debt levels and to isolate the impact of fiscal news on borrowing costs.

The data that we analyze are from a Chubb Corporation survey of state tax-exempt bond yields. Survey respondents are asked to report the *relative*

yield on a general obligation bond issued by state i, compared with a similar bond issued by New Jersey. These data measure $R_{it} - R_{jt}$, where R_{jt} denotes the yield on New Jersey's bonds. The simple yield determination theory in Eq. (1) implies that

$$(2) \qquad R_{it} - R_{jt} = \left[1 - \tau_{f,i}(B_t)\right] R_{Tt} + \sigma_i(Z_{it}, X_{it}, B_{it}, C_{it}) - \left[1 - \tau_{f,j}(B_t)\right] R_{Tt} + \sigma_j(Z_{jt}, X_{jt}, B_{jt}, C_{jt}).$$

Under the assumption that the marginal federal income tax rates of the marginal holders of municipal bonds in different states are the same, the terms in (2) that involve the Treasury bond yield and the federal marginal tax rate will cancel. This yields

$$(3) \qquad R_{it} - R_{jt} = \sigma_i(Z_{it}, X_{it}, B_{it}, C_{it}) - \sigma_j(Z_{jt}, X_{jt}, B_{jt}, C_{jt}).$$

The assumption that $\tau_{f,i} = \tau_{f,j}$ assumes either that there is a single marginal holder of the bonds from all states or that the marginal holders of bonds issued by different states happen to face the same marginal tax rate. There is usually a tax incentive for state residents to hold in-state tax-exempt bonds: many states tax out-of-state ''tax exempt'' interest, although such interest is exempt from federal income taxation, so the second scenario is the more plausible one to assume for our analysis. Given the small number of federal income tax brackets through most of our sample period, this assumption does not seem implausible.

To obtain a form of Eq. (3) that is suitable for estimation, we linearize the risk premium function for each state's bonds as follows:

$$(4) \qquad \sigma_i(Z_{it}, X_{it}, B_{it}, C_{it}) \simeq X_{it} * \alpha_1 + Z_{it} * \beta_1 + X_{it} * Z_{it} * \pi_1 + B_{it} * \gamma_1 + C_{it} * \delta_1 + \varepsilon_{it}.$$

In this expression, we allow for potential interactions between state fiscal institutions and state fiscal and economic conditions, because such interactions are central to the view that bond market participants consider fiscal institutions in evaluating the effect of economic and fiscal conditions. The ε_{it} term in Eq. (4) captures omitted variables and errors of approximation. Substituting (4) into (3) yields an equation that can be estimated using data on the differences between bond yields in different states,

$$(5) \qquad R_{it} - R_{jt} = (X_{it} - X_{jt}) * \alpha_1 + (Z_{it} - Z_{jt}) * \beta_1 + (X_{it} * Z_{it} - X_{jt} * Z_{jt}) * \pi_1 + (B_{it} - B_{jt}) * \gamma_1 + (C_{it} - C_{jt}) * \delta_1 + \varepsilon_{it} - \varepsilon_{jt}.$$

In an earlier study, [24], we estimated regression models like Eq. (5) to explain cross-sectional differences in state bond yields. In the present study, our emphasis is on how bond market participants react to *news* about state fiscal conditions. We are therefore interested in modeling changes over time in the yield differential between a given state's bonds and those of New Jersey. To translate Eq. (5) into an equation that we estimate, we apply a time-difference operator to Eq. (5). This yields

$$\begin{aligned}(6)\quad \Delta(R_{it} - R_{jt}) = {} & \Delta(X_{it} - X_{jt}) * \alpha_1 + \Delta(Z_{it} - Z_{jt}) * \beta_1 \\ & + \Delta(X_{it} * Z_{it} - X_{jt} * Z_{jt}) * \pi_1 + \Delta(B_{it} - B_{jt}) * \gamma_1 \\ & + \Delta(C_{it} - C_{jt}) * \delta_1 + \Delta(\varepsilon_{it} - \varepsilon_{jt}).\end{aligned}$$

State fiscal institutions vary little, if at all, over time. We assume that state fiscal tastes are also time invariant. This permits us to drop two terms in Eq. (6) and to simplify the term involving $\Delta(X_{it} * Z_{it} - X_{jt} * Z_{jt})$. The resulting equation, which is the basis for our empirical work, is:

$$\begin{aligned}(7)\qquad \Delta(R_{it} - R_{jt}) = {} & \Delta(X_{it} - X_{jt}) * \alpha_1 \\ & + (\Delta X_{it} * Z_{it} - \Delta X_{jt} * Z_{jt}) * \pi_1 \\ & + \Delta(B_{it} - B_{jt}) * \gamma_1 + \Delta(\varepsilon_{it} - \varepsilon_{jt}).\end{aligned}$$

While the variables corresponding to economic and fiscal conditions that are included on the right-hand side of Eq. (7) are unlikely to be affected substantially by the yields on newly-issued state bonds, the state debt variable (B_{it}) is potentially endogenous. Ideally, one would find instrumental variables that might influence the change in the state's debt stock, but which would not directly affect the change in the state's borrowing cost. We have yet to identify any such variables. In most of the results that follow, we include the change in the stock of state debt in our estimating equations for the change in borrowing costs. In most cases an increase in the stock of outstanding debt is associated with an increase in borrowing costs, although the exclusion of this variable from the estimating equations does not substantially affect most of the other coefficients.

2. UNEXPECTED STATE FISCAL SHOCKS

Our empirical work follows Poterba [22] in defining budget ''shocks'' that are revealed within a state's fiscal year. Each year, the National Association of State Budget Officers (NASBO) surveys its members and obtains information on actual revenues and expenditures in the last fiscal year. It also collects data on fiscal year revenues and expenditures as budgeted at the beginning of the fiscal year and on budget cuts or tax changes that have been enacted in the

current fiscal year. Virtually all states respond to these surveys. Information on budget cuts is available since the mid-1980s, but data on tax increases have only been collected since 1988. The survey responses can be used to construct measures of state fiscal shocks and to study associated expenditure cuts and tax changes.

Fiscal shocks are measured as follows. The unexpected component of revenues equals the difference between forecast revenues at the beginning of the fiscal year and the revenues that would have been collected during the fiscal year, given actual economic conditions, with the tax system that was in effect at *the beginning of the fiscal year*. State budget officers prepare revenue forecasts as an integral part of each state's dynamic fiscal plan. Their forecasts only apply in a static legislative environment. If the legislature modifies the tax system during the fiscal year, then the difference between actual revenues and the revenue forecast will *not* measure the unexpected revenue shock. If within-year tax enactments raise revenues, then the measured deficit will be smaller than the sum of the projected deficit (if any) and the deficit shock within the year.

To correct for tax changes within the fiscal year, the revenue shock for state i in year t is defined as

$$(8) \quad \text{REVSHOCK}_{it} = \text{Actual Revenues}_{it} - \Delta\text{TAX}_{it} - \text{Forecast Revenues}_{it}.$$

ΔTAX_{it} is the change in revenue during fiscal year t that results from tax changes enacted during that fiscal year. ΔTAX_{it} does not include the effect of previously enacted tax changes that take effect during fiscal year t, since the revenue effect of these anticipated changes should be included in the revenue forecast at the beginning of the fiscal year.

Paralleling this definition of revenue shocks, expenditure shocks are defined by

$$(9) \quad \text{EXPSHOCK}_{it} = \text{Actual Outlays}_{it} - \Delta\text{SPEND}_{it} - \text{Forecast Outlays}_{it}.$$

ΔSPEND_{it} measures any spending changes enacted after the initial budget but during fiscal year t.[2] Combining revenue and expenditure shocks yields the unexpected deficit shock in a given fiscal year,

$$(10) \quad \text{DEFSHOCK}_{it} = \text{EXPSHOCK}_{it} - \text{REVSHOCK}_{it}.$$

[2] NASBO collects data on spending cuts, but not on spending increases, within the fiscal year. Although increases within a fiscal year are less common than budget cuts, this data problem contaminates the estimates of spending changes. If spending needs within a fiscal year raise outlays relative to beginning-of-yer projections, but the state enacts program cuts so that total outlays for the fiscal year equal projected outlays, ΔSPEND_{it} will be negative. Even if actual and forecast outlays coincide in this case, EXPSHOCK_{it} will be positive.

In a later section, we explore the robustness of our findings by developing an alternative measure of unexpected deficits using regression analysis of historical data, rather than the forecasts provided by the state budget offices.

Positive values of DEFSHOCK correspond to unexpected budget deficits, while negative values reflect surpluses. To place these shocks in context, we present two measures of DEFSHOCK. The first is the real per capita value of DEFSHOCK, and the second is DEFSHOCK scaled by the level of projected state revenues for the corresponding fiscal year. We focus on projected revenues primarily because, when we move to the regression analysis below, this variable avoids measurement error problems that might arise if we divide the deficit shock by the actual revenue for the fiscal year.

Table 1 shows the time series evolution of the average deficit shock over the 1988–1999 period. Our regression sample period is shorter than this period; it concludes in 1998 because of limits on the availability of state debt data. The table tracks the changing fiscal fortunes of the U.S. states. In the first half of our sample, the average state experienced an unexpected fiscal shortfall. The largest fiscal shocks relative to revenues were realized in 1991 and 1992. These were the ''fiscal crisis years'' for the states. In more recent years, state finances have grown stronger, with a deficit shock that is negative on average. Negative

TABLE 1

Average State Deficit Shocks, per Capita and Percentage of Projected State Revenue, 1988–1999

Year	Average per capita DEFSHOCK	Average absolute value of DEFSHOCK per capita	Average DEFSHOCK as a percentage of projected revenue	Average absolute value of DEFSHOCK as a percentage of projected revenue
1988	−$49.89	$89.61	−2.15%	5.58%
1989	−$24.73	$60.65	−1.74	4.68
1990	$21.17	$82.28	1.78	5.44
1991	$37.55	$90.86	3.05	5.53
1992	$40.58	$58.72	2.00	3.48
1993	−$19.20	$48.73	−0.82	3.34
1994	−$31.95	$47.21	−2.01	3.46
1995	−$16.00	$48.85	−1.32	3.39
1996	−$20.82	$47.99	−1.51	3.24
1997	−$27.75	$40.76	−2.04	2.96
1998	−$51.52	$76.98	−3.53	4.88
1999	−$21.36	$60.55	−1.62	3.80

Note. Positive values (i.e., 1.28%) correspond to deficits that were larger than expected. Data are based on information reported by the National Association of State Budget Officers and the authors' calculations. Per capita values are measured in real $1998.

deficit shocks correspond to budget surpluses. Unexpected state surpluses averaged more than 1% of revenue between 1993 and 1999. In 1998, the year with the most favorable fiscal news, the unexpected surplus averaged 3.5% of projected revenue. This unexpected surplus was larger than the worst unexpected deficits of the early 1990s.

The average value of DEFSHOCK over the 1988–1999 period is approximately −0.71% of projected general fund revenue. This average includes offsetting effects of positive and negative deficits, however. If we calculate the absolute value of DEFSHOCK relative to the revenues for each state and average the resulting ratios across states, we obtain a more useful measure of the typical size of the unexpected fiscal shock that confronts state budgeters. In 1997, for example, when the average value of DEFSHOCK as a percentage of projected revenue was −2.0%, the average value of the absolute value of DEFSHOCK as a percentage of projected revenue was 3.0%. For the entire 12-year sample we consider, the average value of this ratio is 4.0%. This suggests that unexpected fiscal shocks provide a potentially useful source of variation for studying the determination of state bond yields.

There are important differences across states within our sample period in the average size of unexpected fiscal shocks. Table 2 presents these sample averages, as well as some subsample information. For California, DEFSHOCK as a share of projected revenue averages 1.6%. For New York, this average is 1.8%; Texas, 0.2%; Illinois, 0.2%. Nebraska and Alaska are the states with the largest fiscal windfalls, as a percentage of projected revenue. Revenue dynamics for Alaska are very different from those for the other states in our sample, in that they are highly dependent on fluctuations in oil prices and the associated revenues from natural resource extraction taxes. We have therefore excluded Alaska from the regression analyses that we present below, even though this exclusion does not have a material impact on our results.

Table 2 also shows the average value of state bond yields relative to yields for New Jersey. Ten states have missing values for these data, indicating that they are not included in the Chubb database. There are substantial differences across states in average borrowing costs. Between 1988 and 1999, the estimates suggest that the average borrowing rate for Louisiana was 40 basis points higher than that for New Jersey. For Massachusetts, the analogous statistic is 25 basis points. Other states, including Virginia, North Carolina, and Missouri, have borrowed at lower rates than those for the state of New Jersey. The motivation for our project derives in part from a desire to explain these interstate differences.

The Chubb data are not necessarily collected on a cycle that coincides with state fiscal years. This means that it is possible that some news about state fiscal conditions arrives between two adjacent surveys. However, we have tried to minimize this problem by using data on bond yields collected as close as possible to the start of a new fiscal year.

TABLE 2
Average State Deficit Shock, 1988–1999 Period and Subperiods

State	Average deficit shock, 1988–1999	Average deficit shock, 1988–1992	Average deficit shock, 1993–1999	Average state borrowing cost (relative to New Jersey), 1988–1999
Alaska	−4.19	−12.50	2.73	18.44
Alabama	0.13	1.26	−0.63	9.30
Arkansas	0.37	0.81	0.07	NA
Arizona	−1.36	3.33	−4.71	NA
California	1.55	4.88	−0.35	6.37
Colorado	−0.88	−0.44	−1.20	NA
Connecticut	2.44	8.16	−1.65	2.38
Delaware	−5.92	−1.10	−9.36	5.70
Florida	0.75	3.70	−1.36	8.31
Georgia	−0.76	2.72	−3.24	−4.11
Hawaii	−1.94	−7.47	2.01	12.36
Iowa	1.60	2.95	0.64	NA
Idaho	−2.08	−3.61	−0.99	NA
Illinois	0.15	2.01	−1.18	15.80
Indiana	−1.65	1.07	−3.59	NA
Kansas	−3.51	−3.57	−3.47	NA
Kentucky	0.17	1.68	−0.91	6.41
Louisiana	−2.06	−5.49	−0.10	40.63
Massachusetts	3.62	12.80	−2.95	25.07
Maryland	−0.82	0.24	−1.58	−3.83
Maine	−1.43	2.18	−4.01	7.91
Michigan	1.82	2.90	1.05	10.72
Minnesota	−3.23	−0.73	−5.02	3.47
Missouri	−1.18	2.27	−3.65	−7.33
Mississippi	−1.58	1.49	−3.78	9.70
Montana	−0.77	−1.75	−0.06	4.25
North Carolina	−2.45	0.27	−4.40	−8.61
North Dakota	−0.44	1.46	−1.79	11.28
Nebraska	−5.55	−9.26	−2.90	NA
New Hampshire	−0.62	1.84	−2.38	9.99
New Jersey	−1.31	1.05	−2.99	0.00
New Mexico	−1.19	−0.82	−1.44	9.29
Nevada	1.81	1.89	1.75	15.27
New York	1.81	3.36	0.70	20.73
Ohio	−1.76	0.96	−3.71	0.63
Oklahoma	−0.36	0.09	−0.68	5.82
Oregon	−4.02	−3.10	−4.68	5.67
Pennsylvania	−0.30	1.44	−1.54	12.06
Rhode Island	0.73	3.97	−1.59	16.08
South Carolina	−1.44	1.88	−3.81	−5.48
South Dakota	−0.48	−2.27	0.80	NA

TABLE 2—*Continued*

State	Average deficit shock, 1988–1999	Average deficit shock, 1988–1992	Average deficit shock, 1993–1999	Average state borrowing cost (relative to New Jersey), 1988–1999
Tennessee	0.44	3.05	−1.42	−4.02
Texas	0.24	8.89	−6.97	11.82
Utah	−2.62	−3.25	−2.18	−0.08
Virginia	−1.29	−0.34	−1.97	−7.79
Vermont	3.32	4.09	2.77	9.14
Washington	−1.11	−1.97	−0.49	15.83
Wisconsin	−1.89	−1.96	−1.84	11.43
West Virginia	−0.19	3.99	−3.18	18.69
Wyoming	−1.40	−2.90	−0.32	NA

Notes. Authors' tabulations are based on information reported by the National Association of State Budget Officers. Entries correspond to deficit shocks as a percentage of proposed revenue.

3. STATE FISCAL INSTITUTIONS AND STATE BORROWING COSTS

Many state fiscal institutions may affect state borrowing costs. Briffault [6] explains the working of state budget processes, and he notes that there are many different institutions that impinge on fiscal policy-making. Any aspect of the fiscal constitution or the political environment that affects the future supply of state debt could, in principle, affect debt yields. We focus on three types of fiscal institutions: antideficit rules, limits on the ability of state legislatures to issue debt, and limits on state taxes or expenditures. We select these variables in part because they are relatively straightforward to identify and in part because they bear on the most active policy debates that surround the economic effects of fiscal rules. This section describes these fiscal institutions and our approach to measuring them. It also describes the data on general obligation debt yields that underlie our study.

3.1. *Fiscal Rules*

There is substantial heterogeneity in the first fiscal institution we consider, state balanced budget rules. Only one state, Vermont, does not have a formal balanced budget requirement. The balanced budget requirements in the 49 states with such requirements can be categorized broadly into four groups, depending on the stage in the budget process at which balance is required. In 44 states, the governor must submit a balanced budget. This is the weakest of the various balanced budget requirements. In 37 of these states, the legislature must enact a balanced budget. These balanced budget rules nevertheless allow for actual

revenues and expenditures to diverge from balance if realizations differ from expectations. In six states, any unexpected deficit must be corrected in the next budget cycle. Finally, in 24 of the 37 states which require the passage of a balanced budget, there is a prohibition on deficit carry-forward.

Our data on balanced budget rules are drawn from the Advisory Council on Intergovernmental Relations (hereafter ACIR) [1] report on institutions that promote fiscal discipline in the states, updated using subsequent issues of the ACIR publication *Significant Features of Fiscal Federalism*. The ACIR index of budget stringency ranges between 0 (lax) and 10 (stringent), and we use an indicator variable for whether this index is below 6 in our empirical work below. States with scores below six may have requirements that the governor's proposed budget balance or that the legislature pass a balanced budget. States that require a balanced budget at the end of the fiscal year, however, would score a value of either 9 or 10 on the ACIR scale, so they would not be identified by our indicator variable. Similarly, states that require a balanced budget over a two-year cycle receive an ACIR score of 8 and would not be captured by our indicator variable.

We use the discrete indicator variable for the ACIR score less than six, rather than the actual value of the ACIR score, because the latter imposes the same fiscal effect of one-unit changes at different levels of the ACIR scale, even though these differentials may be noncomparable. The indicator variable that we use, which was also analyzed in Poterba [22], Poterba and Rueben [24], and Bohn and Inman [5], captures the key variation between states with lax and strict budgetary rules, but it is not affected by small differences between states near either extreme. We have tried including states which allow a deficit carry forward into the next budget cycle (ACIR scores of 5 and 6) in both categories and the results do not change substantially.

While an overwhelming number of states require budgets to be balanced during the current year, states in the Northeast and by the Great Lakes are less likely to have stringent requirements. Many of the states outside those regions with less stringent budget rules, such as California, Nevada, and Louisiana, have more recently passed other fiscal constraints that restrict state revenue or expenditures.

The second fiscal institution that we consider is the ease with which the state can issue additional long-term general obligation debt. There is substantial heterogeneity with respect to state debt limits. Ten states have no restrictions on debt issuance; of the other 40, thirty-eight have constitutional restrictions on debt issue and two have legislative limits. The most common restriction places a dollar limit on the amount of debt that can be outstanding; this limit varies from $50,000 in Rhode Island and Oregon to $3 million in Alabama. In 10 states (Arizona, California, Idaho, Kansas, Kentucky, Maine, Missouri, New Jersey, Pennsylvania, and Rhode Island) voters can override the constitutional restrictions on debt levels to issue additional debt. In three other states,

including New York, voters are required to approve any debt issue. Another three states require a supermajority vote in the state legislature. We define an indicator variable for all states with *any* type of debt restriction and include this indicator variable in our analysis below.

The last three fiscal variables that we consider are indicator variables for whether a state has a limit on state taxes, whether a state has a limit on state expenditures, and whether the state requires a supermajority provision for the enactment of new taxes. Tax and expenditure limits, which we refer to as TELs, typically limit the growth rate of general fund expenditures or revenues to that of personal income. Our analysis relies on Rueben's [26] classification of state tax and expenditure limits as ''binding,'' which essentially consists of limits that cannot be overridden by a simple legislative majority. The nature of these rules has been updated using information reported in Rafool [25]. Many limits on the growth rate of taxes or expenditures were enacted during the tax revolt of the late 1970s, although some are more recent and have been passed during the 1990s. Twenty-five states have instituted some form of limitation since 1976.

From the standpoint of the tax-exempt bond market, limitations on revenues and expenditures may have different effects. Limits on the taxing authority of the legislature may increase the risk that future tax receipts will not cover interest payments. Knight [16] reports that when Ohio considered adopting a supermajority requirement for tax increases, the chilling effect of such a rule on state borrowing costs was one of the factors that led to its defeat. Limits on expenditures, however, in many cases do not apply to interest outlays. They may even have a favorable effect on the perceived riskiness of state bonds, since they may constrain the future expenditures that might compete with promised interest payouts as future demands on the state's tax capacity. In contrast, restrictions on a state legislature's power to tax raise the chance that state interest obligations may not be met. Thus we might expect that states with tax limits would face higher borrowing costs than states with expenditure limits.

Table 3 reprints summary information on antideficit rules, debt issue restrictions, and tax and expenditure limits for the 50 states. Our empirical work uses these measures of fiscal institutions to help explain how state bond yields react to news about budget deficits and state economic conditions.

3.2. *State Borrowing Costs*

Our empirical work essentially tries to explain changes in the yield on 20-year general obligation debt issued by state i as reported in the Chubb Insurance Company ''Relative Value Survey.'' This survey, which has been carried out every six months since 1973, asks 20−25 sell-side bond traders at major brokerage houses that deal in tax-exempt bonds to estimate the current yields on general obligation (GO) bonds from 40 states. The states excluded

TABLE 3
State Fiscal Institutions

State	Balanced budget stringency	Debt restriction	Year passed	
			Spending limit	Revenue limit
Alabama	10	yes		
Alaska	6	yes	1982	
Arizona	10	yes	1978	
Arkansas	9	yes*		
California	6	yes*	1979	
Colorado	10	no	1992[a]	1992
Connecticut	5	no	1991	
Delaware	10	no		
Florida	10	yes*		1994
Georgia	10	yes		
Hawaii	10	yes	1978	
Idaho	10	yes	1980	
Illinois	4	no		
Indiana	10	yes		
Iowa	10	yes		
Kansas	10	yes*		
Kentucky	10	yes*		
Louisiana	4	no		1991[b]
Maine	9	yes*		
Maryland	6	no		
Massachusetts	3	no		1986
Michigan	6	yes*		1978
Minnesota	8	yes		
Mississippi	9	yes		
Missouri	10	yes*	1980	1980
Montana	10	none	1981	
Nebraska	10	yes		
Nevada	4	yes	1994[c]	
New Hampshire	2	none		
New Jersey	10	yes*	1976[d]	
New Mexico	10	yes		
New York	3	yes*		
North Carolina	10	none		
North Dakota	8	yes		
Ohio	10	yes		
Oklahoma	10	no	1985	
Oregon	8	yes*		
Pennsylvania	6	yes*		
Rhode Island	10	yes*	1992[e]	
South Carolina	10	yes*	1980	
South Dakota	10	yes		
Tennessee	10	no		

TABLE 3—*Continued*

State	Balanced budget stringency	Debt restriction	Year passed	
			Spending limit	Revenue limit
Texas	8	yes		
Utah	10	yes		
Vermont	0	yes		
Virginia	8	yes		
Washington	8	yes		1979
West Virginia	10	yes		
Wisconsin	6	yes		
Wyoming	8	yes		

Notes. Data on budget stringency rules and debt restrictions are from ACIR [1] and Rafool [25]. Data on revenue and expenditure limits are from Rueben [26].

* denotes states that require a popular vote to approve a debt issue. Nonbinding limits can be overridden with simple legislative majorities.

[a] Colorado passed a nonbinding spending limit in 1977.

[b] Louisiana adopted a nonbinding revenue limit in 1979 and a binding one in 1991.

[c] Nevada passed a nonbinding spending limit in 1979.

[d] New Jersey's spending limit, passed in 1976, expired in 1983.

[e] Rhode Island's nonbinding limit, adopted in 1977, was replaced with a binding limit in 1992.

from the sample—Arizona, Arkansas, Colorado, Idaho, Indiana, Iowa, Kansas, Nebraska, South Dakota, and Wyoming—are concentrated in the Midwest and Great Plains regions. The participants in the Chubb Survey are asked to evaluate ''hypothetical'' general obligation bonds which come due in 20 years, so reported differences in yields should not be attributable to differences in call provisions or other factors, but simply to the perceived riskiness of the state's general obligation debt. For each time period in our sample there is substantial variation across states in the survey-based estimates of borrowing costs. In January 1998, for example, California GO bonds were yielding four basis points more, New York bonds 13 basis points more, and Virginia bonds 6 basis points less than New Jersey bonds. There was a 21 basis point spread between the highest and lowest yield states. There is some evidence of a decline over time in the dispersion of bond yields.

Table 4 reports the yield spread between the highest and lowest yield bonds in the Chubb sample for each year in our sample. This yield differential at the end of the sample is much smaller than the yield spread in earlier years. In June 1990, for example, this yield spread was 84 basis points, while in 1999 it was only 22 basis points. The narrowing of yield spreads reflects the generally stronger fiscal health of states in recent years. It may also reflect the growth of

TABLE 4
Yield Spread Between Highest and Lowest Yield Bonds in Chubb Survey

Year	Maximum−minimum yield spread	Average U.S. yield− New Jersey yield
1988	120.2	14.9
1989	88.5	10.9
1990	73.1	9.1
1991	84.1	14.4
1992	44.1	8.9
1993	44.7	8.0
1994	31.9	5.7
1995	37.6	5.3
1996	35.9	5.6
1997	27.5	4.3
1998	22.0	4.0
1999	22.3	4.6

Source: Authors' tabulations are based on information from Chubb Relative Value Survey. See text for further details.

municipal bond insurance during our sample period. Although the Chubb survey asks respondents to price an uninsured bond, as insurance has become more common it may be more difficult for traders to estimate the yield on such a security. This may be reflected in shrinking reported yield spreads. Table 4 reports data through 1999, but our regression results below are restricted to the 1988−1998 period as a result of the limited availability of data on state debt outstanding.

To investigate the relationship between the Chubb survey data and market-based bond yields, we have compared the relative spreads reported in the survey with actual yields on municipal bonds. We have focused on the relative return on California and New York bonds from Chubb with yields as listed in the Bond Buyer Index of 20 general obligation bonds. The 20-bond index, measuring yields of GO bonds maturing in 20 years, is supposed to have a rating roughly equivalent to Moody's A1. Two of the individual securities considered for this bond index are California and New York state securities. For the period from December 1, 1988, through August 13, 1992, California's yield averaged 38 basis points less than New York's. This compares with 36.5 basis points from the Chubb survey.

There are substantial short-run changes in state bond yields. Although many changes are small, in some cases there are large yield movements. Between July 1989 and July 1990, for example, the yield on Massachusetts GO bonds, relative to New Jersey GO bonds, rose by 38 basis points. It declined by 45 basis points between July 1991 and July 1992, reflecting the rapid changes in both economic conditions and political circumstances in the state. The mini-

mum and maximum changes in values both occurred in Louisiana. Between 1993 to 1994, the relative yield on Louisiana debt declined 69 points. In California the relative yield dropped 27 points between July 1988 and July 1989 and increased by almost 24 basis points between 1991 and 1992. These statistics suggest important state-specific variation in general obligation borrowing costs.[3]

4. EMPIRICAL FINDINGS

The substantial within- and across-state variation in bond yields is the basis for our empirical work. We focus on the change, between two Chubb surveys, in the relative bond yields for state i. We relate this difference to the deficit shock (DEFSHOCK) between these two surveys, and then we interact this DEFSHOCK difference with measures of fiscal institutions. Thus, the equations that we estimate take the form

$$
\begin{aligned}
(11)\quad \Delta\left(R_{it} - R_{jt}\right) = {} & \left(\text{DEFSHOCK}_{it} - \text{DEFSHOCK}_{jt}\right) * \alpha_1 \\
& + \Delta\left(RU_{it} - RU_{jt}\right) * \alpha_2 \\
& + \left(\text{DEFSHOCK}_{it} * Z_{it} - \text{DEFSHOCK}_{jt} * Z_{jt}\right) * \pi_1 \\
& + \Delta\left(B_{it} - B_{jt}\right) * \gamma_1 + \Delta\varepsilon_{it}.
\end{aligned}
$$

The central question of interest is whether the bond market's reaction to an unexpected deficit shock depends on the set of fiscal institutions in place, the Z_i variables. We divide DEFSHOCK and the change in state debt outstanding by projected revenue to scale these variables to constant units across states.

The impact of DEFSHOCK on the change in the yield is partly due to the fact that DEFSHOCK is affected by economic conditions. These economic conditions may affect yields directly. To control for the ''pure economic condition'' effect associated with DEFSHOCK, we include the change in the state's unemployment rate (UR) in some of our estimating equations. We have also estimated similar models by scaling the explanatory variables to per capita units rather than by projected revenues. The results are similar to those reported below.

[3] We have studied the time series properties of changes in the Chubb survey's state bond yields. A natural first question is whether the reported yields follow a random walk or whether their changes are partly predictable. Using tests for the presence of unit roots in panel data outlined in Harris and Tzavalis [14], we rejected the null hypothesis of a unit root in bond yields. However, when we applied Dickey–Fuller [8] tests separately on a state-by-state basis, we rejected the null hypothesis of a unit root at the 95% confidence level for only two states (Georgia and Rhode Island), and for 10 states at the 90% confidence level. Taken together, these results suggest some ''mean reversion'' in these yields, possibly reflecting the fact that these bond yields are not derived from actual asset values in a financial market.

Table 5 presents the results from regression models relating the change in the state bond yield to the change in state budgetary conditions. The estimates in the first column show that an unexpected increase in the state deficit has a positive but statistically insignificant effect on the state's bond yield. That is, if the reported deficit is projected to be larger, the bond yield will rise. A deficit shock of 5% of projected revenue, which is slightly less than a one standard deviation change (6.6%) in this variable, would result in a 69 basis point increase in the state bond rate. The next two columns show the effect of including control variables, the change in the state unemployment rate and the change in state debt, in the specification. An increase in the state unemployment rate has a positive and statistically significant effect on the state bond yield, and so does an increase in state debt outstanding. Including these variables does not have a substantial effect on the estimated coefficient on the unexpected deficit variable. We have tested, in equations not reported here, for differences between the bond market reactions to positive and negative deficit shocks. We cannot reject the null hypothesis of equal reaction to the two types of shocks. The results in Table 5 suggest that deficit shocks may affect bond yields, but they do not provide overwhelming evidence. This may reflect different effects of unexpected deficits in different states, a possibility that we now consider.

Table 6 presents our first results interacting the deficit shock variable with the indicators of fiscal rules. The first column shows that when we interact our indicator variable for weak fiscal rules with the deficit shock, the interaction

TABLE 5

The Effect of Unexpected Deficits on Changes in Relative State Bond Yields

Explanatory variable				
Change in state unemployment rate		1.84 (0.56)	1.79 (0.56)	
Unexpected deficit as a percentage of projected revenue (DEFSHOCK)	0.138 (0.08)	0.11 (0.06)	0.11 (0.07)	0.14 (0.08)
Change in state debt outstanding as a percentage of projected revenue		0.03 (0.01)		0.02 (0.01)
Constant	−1.29 (0.29)	−0.92 (0.23)	−0.90 (0.24)	−1.32 (0.29)
Adjusted R^2	0.024	0.106	0.097	0.029

Notes. Data are for 1988–1998 for the 39 states, excluding Alaska, covered by the Chubb Survey. Year effects are included in each regression equation; there are a total of 411 observations. Standard errors, which are shown in parentheses, control for across-state heterogeneity and within-state correlation using a procedure suggested by Moulton [19].

TABLE 6

The Effect of Unexpected Deficit Shocks and Fiscal Institutions on Changes in Relative State Bond Yields

Explanatory variables						
Change in state unemployment rate	1.45 (0.35)	1.72 (0.44)	1.72 (0.40)	1.51 (0.35)	1.48	
Change in state debt as a percentage of projected revenue	0.03 (0.01)	0.02 (0.01)	0.02 (0.01)	0.02	(0.01)	0.02
Unexpected deficit as a percentage of projected revenue (DEFSHOCK)	−0.01 (0.05)	0.49 (0.19)	0.06 (0.07)	0.04 (0.14)	0.07 (0.14)	0.01 (0.15)
DEFSHOCK*Lax antideficit rules	0.65 (0.21)			0.42 (0.16)	0.40 (0.16)	0.52 (0.19)
DEFSHOCK*Limit on issuing debt		−0.49 (0.21)		−0.07 (0.12)	−0.10 (0.12)	−0.04 (0.13)
DEFSHOCK*Binding expenditure limit			−0.12 (0.12)	−0.05 (0.10)	−0.06 (0.10)	−0.02 (0.11)
DEFSHOCK*Binding revenue limit			0.74 (0.26)	0.44 (0.22)	0.46 (0.24)	0.46 (0.24)
DEFSHOCK*Supermajority tax provision			0.40 (0.21)	0.34 (0.20)	0.34 (0.20)	0.33 (0.20)
Constant	−1.10 (0.22)	−0.80 (0.24)	−0.72 (0.25)	−0.90 (0.23)	−0.86 (0.24)	−1.29 (0.26)
Adjusted R^2	0.216	0.181	0.212	0.249	0.199	0.246

Notes. Data are for 1988–1998 for the 39 states covered in the Chubb survey excluding Alaska. There are 411 observations. Annual indicator variables are included in each regression. Standard errors, which are shown in parentheses, control for across-state heterogeneity and within-state correlation using a procedure suggested by Moulton [19].

variable enters with a statistically significant and substantively large positive coefficient. The coefficients imply that a deficit equal to 5% of projected revenue hardly affects borrowing costs in a state with tight antideficit rules, while it raises borrowing costs by 3.3 basis points $(.05*(0.65 - 0.01))$ in a state without such rules. The second column of Table 6 presents results with an interaction between the unexpected deficit and state limits on issuing debt. In states without limits on debt, the effect of a 5% unexpected deficit shock is a 2.5 basis point increase in the borrowing rate. If the state has strict limits on debt issue, the effect of the unexpected deficit shock virtually disappears.

The next column in Table 6 includes interactions of indicator variables for binding revenue and expenditure limits, and supermajority requirements, with the unexpected deficit shock. The results suggest that an unexpected increase in the state deficit has a much larger effect on bond yields in a state with a binding revenue limit than in a state with a binding expenditure limit. The results in

column three suggest that for a state with a binding expenditure limit there is almost no effect from an unexpected deficit on the borrowing rate. A 5% of projected revenue DEFSHOCK in a state with a binding revenue limit, however, translates into a 3.5 basis point increase in borrowing rates. This is a much larger effect than the 0.3 basis point increase that we find for states that have neither tax nor expenditure limits.

The fourth column of Table 6 includes interactions of all four fiscal institution variables with the unexpected deficit. As the earlier results suggest, the effects are most pronounced in states that have weak antideficit rules, according to the ACIR [1] categorization, or that have binding revenue limits. The various fiscal indicators are collinear, so it is difficult to estimate separate effects for antideficit rules, borrowing limits, tax limits, and expenditure limits. The coefficient on debt limits changes most significantly between the ''univariate'' and ''multivariate'' regressions: it is no longer statistically significant in the estimates in the fourth column of Table 6.

The last two columns in Table 6 present results that offer insight on the robustness of our findings. The specification in the penultimate column omits the change in state debt, and that in the last column omits the change in the state unemployment rate. The coefficients on weak antideficit rules and on binding revenue limits, interacted with the deficit shock, change very little in either of these cases.

The results in Table 6 clearly suggest that one of the reasons that DEFSHOCK does not have a statistically significant effect on yield changes when we estimate the equations in Table 5 is because there is heterogeneity across states in the link between deficit shocks and borrowing costs. The empirical results suggest that state differences in fiscal rules are correlated with borrowing costs. The point estimates of these effects, however, are relatively small. Although our regression analysis uses state debt outstanding relative to projected revenue as an explanatory variable, to illustrate the importance of borrowing costs it is more informative to work in terms of debt per capita. At the midpoint of our sample, in 1994, the Advisory Council on Intergovernmental Relations [2] reported that the average per capita state debt outstanding was $2011. In some states the debt burden is several times this level. Using the average value, however, a four basis point change in state borrowing costs would result in a $.0004 * 2000$ change in per capita state outlays, or about 80 cents per year. This seems like a modest effect, although it would cumulate as state residents paid higher interest rates in the current and all future years.

Most of the equations that are reported in Tables 5 and 6 include the change in the state unemployment rate as a control variable for changes in state economic conditions. Including this variable is designed to reduce the explanatory power of unexpected deficits in accounting for changes in borrowing costs, because deficit shocks in part reflect changing economic circumstances. A 1% increase in a state's unemployment rate raises borrowing costs by between 1.45

and 1.85 basis points, depending on the particular equation we consider. In all cases the change in the unemployment rate has a statistically significant effect on borrowing costs, although the quantitative importance of the effect is surprisingly small. These results provide clear evidence that bond market participants are concerned about some aspect of future repayment prospects on state debts, and they suggest that changes in the state unemployment rate can have substantively important effects on the state interest costs imposed on state residents.

To further explore the robustness of our findings, we carried out three further tests. First, we allowed for borrowing costs to be affected by interactions between changes in the state unemployment rate and fiscal rules. This amounts to augmenting Eq. (11) to allow for further interaction terms: i.e.,

$$
\begin{aligned}
(12)\quad \Delta(R_{it} - R_{jt}) = {} & (\text{DEFSHOCK}_{it} - \text{DEFSHOCK}_{jt}) * \alpha_1 \\
& + \Delta(RU_{it} - RU_{jt}) * \alpha_2 \\
& + (\text{DEFSHOCK}_{it} * Z_{it} - \text{DEFSHOCK}_{jt} * Z_{jt}) * \pi_1 \\
& + (\Delta RU_{it} * Z_{it} - \Delta RU_{jt} * Z_{jt}) * \pi_2 \\
& + \Delta(B_{it} - B_{jt}) * \gamma_1 + \Delta \varepsilon_{it}.
\end{aligned}
$$

Table 7 shows the results of estimating (12). Once again, we find that the pattern of coefficients on the interactions between DEFSHOCK and the various fiscal rules is not affected by the inclusion of interactions between the unemployment rate and these rules. There are interesting patterns, however, in the coefficients on the unemployment shock–fiscal rule interactions. An increase in the unemployment rate raises a state's borrowing cost more if it has lax antideficit rules, or a binding expenditure limit, than if it does not. An unemployment shock has a smaller effect in states with limits on issuing debt than in other states.

The findings in Table 7 on the interactions between fiscal variables and deficit shocks and the interactions between fiscal variables and state unemployment shocks suggest a common pattern. In states where fiscal institutions make it more difficult for a current deficit shock to raise future deficits and future state indebtedness, current borrowing costs rise less than in states where the long-run effects of the deficit shock could be an increase in state borrowing.

We describe, but do not present, the empirical results from our other two robustness checks. In the first, we added the indicator variables for weak antideficit rules and other fiscal institutions to the basic estimating equation, while retaining the interaction between these variables and the variable measuring fiscal shocks. Adding these ''level effects'' for the fiscal rules had very

TABLE 7

The Effect of Unexpected Deficit Shocks, Unemployment Shocks, and Fiscal Institutions on Changes in Relative State Bond Yields

Explanatory variables			
Change in state debt outstanding	0.02	0.02	0.02
	(0.01)	(0.01)	(0.01)
Change in state unemployment rate	1.39	3.06	0.68
	(0.33)	(0.70)	(0.48)
Change in unemployment rate* lax antideficit rules	1.16		
	(0.43)		
Change in unemployment rate* limit on issuing debt		−1.53	
		(0.50)	
Change in unemployment rate* binding expenditure limit			1.48
			(0.46)
Change in unemployment* binding revenue limit			1.49
			(1.61)
Change in unemployment rate* supermajority tax			1.28
			(0.90)
Unexpected deficit (DEFSHOCK)	−0.00	0.38	0.10
	(0.05)	(0.17)	(0.08)
DEFSHOCK*lax antideficit rules	0.54		
	(0.20)		
DEFSHOCK*limit on issuing debt		−0.37	
		(0.19)	
DEFSHOCK*binding expenditure limit			−0.17
			(0.12)
DEFSHOCK*binding revenue limit			0.64
			(0.23)
DEFSHOCK*supermajority tax			0.32
			(0.17)
Constant	−1.05	−0.81	−1.00
	(0.22)	(0.24)	(0.25)
Adjusted R^2	0.219	0.191	0.234

Notes. Data are for 1988–1998 for the 39 states covered in the Chubb survey excluding Alaska. There are 411 observations. Standard errors, which are shown in parentheses, control for across-state heterogeneity and within-state correlation using a procedure based on Moulton [19].

little impact on the coefficients on the interactions between fiscal rules and deficit shocks, so we do not report the findings.

Second, we replaced our measure of the deficit shock with an alternative measure of each state's unexpected fiscal conditions. Our foregoing analysis defines ''budget news'' as the difference between the actual budget deficit and the state budget office's forecast. Yet the forecasts generated by the budget bureaucracy may not be unbiased, and they may not be based on all the available information at the time of the forecast. Feenberg and Rosen [11] present some evidence of systematic bias in state deficit projections. If the state

budget forecasts that we study can be refined by the use of other publicly available information, then it is not clear that tax-exempt bond market participants would be surprised by divergences between these forecasts and actual budget outcomes.

To explore this issue, we constructed a simple measure of the expected state deficit for each year in our sample. We did this using a ''rolling regression'' procedure, in which the sample period over which models are estimated varies from one year to the next within our sample. We use all available prior data for each year; this means that the sample size for the regressions that we use to form predictions is longer for the final years in our sample than for the earlier ones. In the rolling regression procedure, we estimated regression models linking state deficits to twice-lagged state revenue, twice-lagged state expenditures, the twice-lagged state unemployment rate, a state fixed effect, and a state-specific time trend. We focus on twice-lagged variables because most states predict their revenues and expenditures one year in advance and therefore would have no more than twice-lagged information available.

We defined the unexpected deficit as the one-period-ahead prediction error from the estimated rolling regression models. We then replaced DEFSHOCK in Eq. (11) with this alternative measure of a state's unexpected fiscal news. The results were surprisingly similar to those reported in our tables. For example, the estimates corresponding to DEFSHOCK estimates in the second column of Table 5 generated a coefficient of 0.082 (0.047) on the unexpected deficit variable. This is somewhat smaller than the coefficient on DEFSHOCK (0.11), but not statistically significantly different. When we added interactions with fiscal institutions, the results using the new measure of the unexpected deficit were usually similar to the DEFSHOCK-based estimates. For example, the estimates corresponding to the equation in the first column of Table 6 yield a coefficient estimate of 0.54 (0.21) on the interaction between the new deficit shock and the indicator variable for weak antideficit rules. The estimates shown in Table 6 are 0.65 (0.21). The only substantial difference that we found when comparing equations estimated with DEFSHOCK and our alternative deficit measure concerned the interaction of expenditure limits and the unexpected deficit variable. In this case, the coefficient using the alternative deficit surprise measure was several times larger than that using the DEFSHOCK variable.

5. CONCLUSIONS AND FUTURE DIRECTIONS

This paper presents new evidence on the economic effect of state fiscal institutions. Previous studies have found that the presence of various fiscal rules, such as tax or expenditure limits, affects the average borrowing costs that states face. Our analysis seeks to avoid the potential endogeneity of fiscal rules by studying how the tax-exempt bond market reacts to *changes* in state fiscal conditions. We present clear evidence of a link between unexpected changes in state deficits and surpluses and changes in the required yield on general

obligation debt. Moreover, the presence or absence of various fiscal rules affects the impact of unexpected deficits on yields. For example, the presence of a limit on the state's capacity to raise taxes is associated with a larger increase in borrowing costs when the state reports an unexpected deficit. These findings provide corroborative evidence on the economic importance of state fiscal rules.

One important issue that we have not explored in any detail concerns the role of factors other than fiscal rules in explaining the bond market's reaction to fiscal news. In results that are not reported here, we have estimated separate regressions for each state relating the change in borrowing costs to DEFSHOCK. These equations have only 11 time-series observations, and the coefficient estimates are necessarily imprecise. They nevertheless suggest substantial differences across states in the effect of an unexpected deficit on borrowing costs. The state-to-state coefficient differences are much larger than the differences suggested by our fiscal institution interactions, and these findings therefore suggest that there are potentially important omitted variables that have not been included in our analysis. Modeling the other factors that may affect the bond market's reaction to state economic news and then developing empirical evidence for or against such models are important directions for future work. Case studies of actual changes in state fiscal conditions and the associated response of the tax exempt bond market could be useful starting points in this analysis.

A second issue that would bear further analysis is the source of fiscal shocks. We have treated state fiscal shocks as though they are exogenous shocks that are beyond the control of state budget officers and elected officials. It is nevertheless possible that the degree of attention that states devote to revenue and expenditure forecasting may be affected by the perceived consequences of forecast errors. In states with tight fiscal rules, which do not experience large increases in borrowing costs when there are unexpected deficits, this may result in a smaller incentive to make accurate forecasts. We have just begun to investigate the correlation between budget rules and the accuracy of budget forecasts.

A third issue that bears investigation is the extent to which fiscal crises, reflected in large values of the DEFSHOCK variable, result in ''discontinuous'' changes in fiscal policy. It is possible that situations such as fiscal crises that require substantial changes in state expenditure levels or state tax policies create a political dynamic that is different from the dynamic that leads to routine fiscal decisions. Such shocks could, for example, change the identity of agenda setters within the state political system, because they may require unusual legislative actions. If such discontinuous changes sometimes take place, then bond market participants may consider this possibility in reacting to news about state fiscal conditions. This suggests that the reaction to fiscal news might be nonlinear or might depend in subtle ways upon the state political

environment. As the span of data on state fiscal conditions and bond yields continues to expand, particularly if the recent fiscal strength of states gives way to another period of fiscal crises, it may be possible to test for such effects.

Finally, it would be useful to investigate systematically the rationality of state budget forecasts of both expenditures and revenues. There may be political influences on these forecasts, and the extent to which these forecasts make use of all publicly available information that could bear on the prediction problem is unclear. Future work could also study links between the fiscal rules, the associated costs of making inaccurate budget forecasts, and the precision of revenue and expenditure predictions.

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